

THURSDAY, MARCH 3, 1881

NATURAL CONDITIONS AND ANIMAL LIFE

The Natural Conditions of Existence as they Affect Animal Life. By Karl Semper, Professor in the University of Würzburg. International Scientific Series. (London: Kegan Paul and Co., 1881.)

THIS is in many respects one of the most interesting contributions to zoological literature which has appeared for some time. The author is well known as an accomplished anatomist and microscopist who, after spending some years in exploring the fauna of the Philippine and neighbouring islands, returned to Europe, and having been appointed to the Chair of Zoology in Würzburg, set himself to work at the morphological problems which so largely occupy at present the attention of anatomists. His most remarkable productions in this department have been his speculations and observations on the segmentation of animals and on the origin of the vertebrate kidney. But Prof. Semper has the advantage of being something more than an anatomist; as a traveller and one who has seen and studied life under most varied conditions, he has thought much and collected many facts bearing upon the problem of the influence of changed conditions of life in modifying the structure of animals submitted to those conditions.

With the leading theoretical consideration advanced by Prof. Semper no naturalist who knows the history of evolutionary theory will agree, but the large collection of well-described and well-illustrated facts for which he claims attention in consequence of his theoretical preconceptions, are none the less interesting. The book has the great merit of being one which will be found equally readable by the professed zoologist and by the general reader.

Prof. Semper, whilst accepting the doctrine of the origin of new forms of life by the natural selection of fittest varieties of pre-existing forms, is unable to conceive of the "fittest varieties" in question, being such slightly divergent forms as are normally to be found in the offspring of all parents. Though he does not explicitly deny the physiological importance of even such minute variations as are not readily perceived by the human eye, and consequently does not openly controvert Mr. Darwin's theory to the effect that such of these minute variations as are fitted to given conditions of existence, are perpetuated and intensified by the survival of those animals in which they occur, and the failure and death of those in which they do not occur, yet Prof. Semper is among those who look for a more rapid and conspicuous method of the production of new species than that taught by pure Darwinism. He thinks that Mr. Darwin has overlooked or underrated the importance of "*directly-transforming* agents." He is no doubt aware that it is equally possible to over-estimate the importance of such action, and that this was done by Mr. Darwin's predecessors. Accordingly he examines in the volume before us such cases as may tend to give evidence on the subject.

Such cases are to be found when an animal living upon special food, or in given temperature, or light, or in water (still or running, fresh or saline), or air (dry or moist, still or breezy), or in isolation, or as parasite, is

subjected to a change in those conditions either by natural processes or by experiment. A large series of natural instances are afforded by pairs of representative species of one genus, the one living under one set of conditions, the other under conditions in which the factor, the influence of which is sought, is removed or altered. Very few experiments, as Prof. Semper remarks, have been made upon this subject, but some of remarkable interest are cited.

The result of the examination of the instances which have been gathered together in this volume is *not* such as to lead to the conclusion that directly transforming agents play an important part in the production of new species. "Changed conditions," Mr. Darwin has said, "induce an almost indefinite amount of fluctuating variability, by which the whole organism is rendered in some degree plastic," and it is to the non-significant variations so produced which are selected by survival and fixed by heredity that new forms are due, and not to those *direct* adaptations effected in the individual by changed conditions, which are remarkably rare, and moreover, as Prof. Semper recognises (p. 38), are not transmitted, as a rule, to offspring. In order to establish his point Prof. Semper should have been able to give us, firstly, numerous instances of change of structure in the individual brought about in adaptation to a change in that individual's conditions of life. He produces very few, whilst the most striking and numerous facts which he records are instances of physiological adaptation to or toleration of new conditions *without any corresponding change of structure*. Secondly, he should have been able to give instances of the transmission to offspring of peculiarities acquired by the parent by undoubted action of the environment on the individual parent. Such instances are excessively rare, though a few are on record; but none are cited by Prof. Semper, and indeed the evidence as at present before us is such as to warrant the conclusion that such transmission cannot be in any way an important factor in the production of new races.

In his concluding paragraph (p. 405) Prof. Semper states that "there is a universal difficulty of deciding whether a modification which has taken place is to be ascribed to some direct determining and modifying cause, or to the enhancing of a previously modified character which is frequently connected with selection," and then deprecates the habit of theoretical explanations from general propositions. He holds apparently that we are not to seek an explanation of such modifications in those truths of heredity and adaptation, of variation and selection, which have been actually demonstrated and established by Mr. Darwin, but must, if we would behave as right-minded philosophers, keep before us the possibility of these modifications being due to—what? Not to a cause which has been shown to be necessarily or even usually at work, as have those to which Mr. Darwin points, but to a cause which has always proved illusory, namely, the "*directly-transforming*" action of the environment. It was because they appealed to this cause and could not show that it had a real existence that the "transformists" of the beginning of this century failed, where Mr. Darwin, appealing to another cause which he showed was an existing cause, has succeeded. Prof.

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Semper's contribution to the subject does not tend to alter the low estimate which has been formed of the efficiency of directly-transforming agents, nor to justify the "final warning" which closes his book. It is then as a repertory of physiological facts of a kind usually neglected both by the professed physiologist and by the professed zoologist that this book will be found of value, not as the expository of new or of old theory.

After an introduction in which, amongst others, some interesting observations on the casting of the skin of reptiles and of crayfish are given with illustrative cuts, we find a chapter on "Food and its Influence." The variety of mineral and organic substances which constitute the food of animals is noted, and monophagous and polyphagous animals distinguished; curious adaptations to a special food such as that of egg-eating snakes, with their gastric teeth formed by processes of the vertebræ, are cited, and some remarkable examples of change of diet naturally occurring in a species *without any modification of structure*, e.g. the New Zealand parrot, which used to feed on the juices of plants and flowers, but now sucks the blood of sheep. Again, horses eating pigeons, vegetivorous snails (*Lymnæus*) eating young newts, crocodiles, some eating men, and others of the same species not prone to the habit. The only well-established instances of modification of structure caused by change of food are due to John Hunter, who fed a gull for a year on grain, and so hardened the inner coat of the bird's stomach as to make it resemble the gizzard of a pigeon; whilst Dr. Holmgren is cited as having obtained the converse result by feeding a pigeon on meat. The change brought about here is, however, not strictly speaking a change of structure, but rather a modification of the chemical activity of the gastric epithelium.

Many instances of wide difference of diet in closely allied species of animals not accompanied by any corresponding difference of structure are given in the text and in the valuable notes at the end of the book.

The influence of light is next discussed, and we have some statements as to the difference in their relation to light, of plants and animals. Prof. Semper does not admit the presence of chlorophyll in any animal, and goes so far as to say that the similarity of the spectrum of the solution of the green pigment of an animal with that of chlorophyll would not prove the pigment to be chlorophyll. If by "similarity" exact correspondence is meant, we should differ from him; but it is no doubt true that further exact observation is needed of those cases among invertebrate animals in which chlorophyll has been supposed to be present.

Semper holds that there is a high degree of probability in the view that the green-coloured bodies present in some lower animals in such abundance are really parasitic Algæ like the gonidia of lichens. As an argument in favour of this view he adduces Max Schultze's observation that the "chlorophyll-bodies" of the worm *Vortex viridis* divide and multiply spontaneously, which he states (in opposition to the generally received observations of Nägeli and the statements of his colleague Sachs) the chlorophyll bodies of plants do not. It would be interesting if this should prove to be the case, and if Prof. Semper should be destined to reform our notions of Vegetable histology among other things.

In a note Semper attacks Paul Bert for saying that "Infusoria containing green matter decompose carbonic acid in the same way as vegetable cells." The French physiologist is well within the facts, for Priestley's green matter was the Flagellate *Euglena viridis*.

It is necessary to point out that it is by no means proved by Cienkowski's observations that the yellow cells of Radiolaria are parasitic one-celled Algæ, as Semper assumes, though it is possible that such is their nature.

Light affects animals mostly through the eye only, and its intensity undoubtedly has a modifying influence upon that organ; but whether the degeneration of the eye in cave animals and deep-sea Fishes and Crustacea is due *directly* to disuse in any instance or to altered selection and heredity, is not clear. Many important facts and some good drawings bearing on this matter are given. Dr. Hagen informed the author that in all the species of cave-beetles of the genus *Machærites* the females *only* are blind, while the males have well-developed eyes, although both live together in total darkness, whilst it is well known that many blind animals, e.g. certain Mollusks, Crustacea, and Worms, live in bright daylight.

Facts are cited showing that the colours of animals are not developed by or dependent on light, whilst the change of colour effected by cuttle-fish, fishes, and Amphibia when light acts on the eye are discussed at length, and the researches of Lister and of Pouchet cited. Prof. Semper, in common with other naturalists, explains the difficulty presented by the colouration of some animals, such as those which live in the dark (many marine polyps and worms), by the assumption that the pigment is the inevitable secondary product of some indispensable physiological process. The same explanation is applied to the phosphorescent material of many marine organisms, which is apparently useless or even injurious to the animals which produce it.

Temperature affords subject-matter for a chapter, abounding in important records of fact, which are, it must be admitted, quite antagonistic to the notion that variations in the environment in this respect can directly produce *adaptive* change of structure. The most remarkable instance of temperature effecting a change of structure is that quoted from Weissman, who, by artificially lowering the temperature, succeeded in rearing *Vanessa levana* from the eggs of *Vanessa prorsa-levana*, the two supposed "species," being only winter and summer varieties of one. But here, though the colouring is different in the two varieties, there is no adaptational character about it, nor a transmission of the changed colouring to offspring.

A number of facts are cited as to the supposed change of colour of Arctic animals in winter, but the conclusion seems to be that no such change occurs. Facts establishing the possibility of freezing whole fish and other animals are given, and other facts showing that 5° below 0° C. kills the tissues of such animals as frogs, and may thus cause death to the whole animal. Important researches of Horvath are cited, showing that the Ground-squirrel (*Spermophilus*), the temperature of whose body is in summer like that of man, about 38° C., can, during its winter sleep, sink to as low a temperature as 2° C. without injury; its body in fact has, at this period, the same temperature as that of the surrounding air. The

rabbit, on the other hand, is infallibly killed when the temperature of its body is reduced to 15° C. The glacier flea (*Desoria glacialis*, one of the Thysanura) is cited as an example of an animal taking up by preference, as it were, a permanently cold life-arena; whilst as examples of endurance of high temperatures we have Crustaceans found in hot springs of 60° C., and fish (*Sparus*) in hot springs of 75° C. The acclimatisation of Mr. Buxton's parrots in Norfolk is described at length, and amongst many other details of the kind concerning the influence of temperature on the spawning and hatching of eggs of various animals, the fact is recorded that at 10°·5 C. the common frog requires 235 days to pass from the egg through complete metamorphosis, whilst at 15°·5 C. only 73 days are required. "Nothing in the Philippine Islands struck me so much," Prof. Semper writes, "as to observe that there all true periodicity had disappeared even from insects, land mollusks, and other land animals; I could at all times find eggs, larvæ, and propagating individuals, in winter as well as in summer." An important reflection in this connection is the following:—"It is generally assumed that we are justified in attributing to extinct animals a mode of life analogous to that of the nearest related surviving forms; . . . as soon as we reach the deeper strata, and the identity of the species with those now living ceases, our right to construct a theory of the climate of past epochs by a comparison of fossil and living species, absolutely disappears." How far, it may well be asked, is this true when plants are substituted for animals?

In a chapter on "The Influence of Stagnant Water" we have a large series of interesting facts and records of experiment under the headings "Freshwater Animals that Live in the Sea" and "Marine Animals in Fresh Water." In both these categories we find a number of animals, whilst as a matter of experiment it is found that, though very few animals will endure sudden transference from fresh to saline water, or *vice versa*, yet a large number will tolerate the change if it be accomplished by slow degrees, whilst others will not endure it, however brought about. The same effect of gradation is noted with regard to change of temperature. But in neither the one case nor the other is Prof. Semper able to cite an instance which tends to favour the view that direct modification of structure is produced by such changes of life conditions.

The instances cited, though not so distinguished by Prof. Semper, may be divided into those afforded by certain species living in one kind of water (fresh or salt), whilst the other species of the genus live in the other kind of water; and secondly, those afforded by exceptional individuals naturally found in one kind of water, whilst normally the individuals of the *same* species occur in the other kind of water. Results derived from the experiment of gradual transference from one kind of water to the other would form a subdivision under this second head. The rare instances of animals living in brine may also be classified in the same manner. Many species allied to river-worms and earth-worms (*Oligochaeta*) are now known to occur in the sea; also Crustacea allied to freshwater forms. Sea-insects and sea-spiders (like the common fresh-water diving spider) are cited in the valuable list of references given at the end of Prof. Semper's

book, and such characteristically fresh-water mollusks as *Cyclas*, *Unio*, and *Anodonta* (found living in the Livonian Gulf with *Telluria* and *Venus*). *Paludina* and *Neritina* are found living in the Caspian with *Mytilus* and *Cardium*: *Planorbis glaber*, in 1415 fathoms in the Mediterranean. Many freshwater species of fishes are recorded from marine waters, and the whole group of sea-snakes form an example in point.

Of marine animals living in fresh-water we have, besides the polyp, *Cordylophora lacustris* (of which some interesting facts, showing its historical advance into fresh-waters, are given by Prof. Semper), and the new jelly-fish *Limnocoedium*, and other jelly-fish and polyps living in estuarine conditions (see *Quart. Journ. of Microsc. Science*, October 1880, for observations by Agassiz and Moseley), some Bryozoa of marine affinities, e.g. *Membranipora*, some Nemertines, and one cephalo-branchiate Annelid, numerous Crustacea, such as *Balanus*, *Mysis*, *Palæmon*. Among Mollusks *Pholades* and *Teredines* are recorded from fresh-water, their congeners being marine, whilst actual marine species of fish (the grey-mullet and the basse) have been bred successfully for the market in the fresh-water Lake of Acqua, near Padua. The common stickleback, as is well known, can be kept in a marine aquarium. Migratory fish such as the salmon are further examples.

The experiments of Beudant and Plateau on the influence on animals of the change of saline to fresh-water or *vice versa* are given in detail, and both are of great interest. Beudant's experiments were made with two series of molluscs—a fresh-water series transferred to salt-water, and a salt-water series transferred to fresh-water. The Pulmonata and species of *Paludina* were found to be very tolerant of sea-water, whilst *Unio*, *Anodonta*, and *Cyclas* were all eventually killed by it. *Patella vulgata*, *Purpura lapillus*, *Arca barbata*, *Venus maculata*, and *Ostrea edulis* survived in large proportion the gradual transference to absolutely fresh-water, whilst of *Mytilus edulis* not a single specimen died in the course of the experiments; species of *Fissurella*, *Haliotis*, *Buccinum*, *Tellina*, *Pecten*, and *Chama* were, on the other hand, killed by the same process.

For full reference to sources of information on this and all the many interesting observations recorded we must refer the reader to Prof. Semper's book.

In successive chapters we have similar details as to the influence of dry air, of currents of water, and of change of life from aquatic to terrestrial conditions; the land leeches, land planarians, land crabs, and land fishes being described and sometimes figured.

Some very remarkable observations on pulmonate snails living in the Lake of Geneva made by M. Forel and by Dr. Pauly are given at length on pp. 197, 198. Certain *Lymnæi* live at great depths in the lake with their lung-sac filled with water; they never come to the surface, and actually breathe water all their lives; but if brought to the surface they take air into the lung-sac and will not again return to the submerged existence. If forced to do so they retain air in their lung-sac and breathe water by the general surface of the body. "In no single case," Prof. Semper frankly observes, "have we as yet succeeded in proving that such a change of function as is involved in the transformation of a gill-

cavity into a lung must necessarily be accompanied by definite changes in the structure of that organ."

After chapters expounding Prof. Semper's original observations and special theory as to the formation of coral islands, in which he characteristically seeks to improve upon Mr. Darwin, and a chapter upon the influence of parasitism, we come to a final chapter entitled "The selective influence of living organisms upon animals." Here new facts bearing upon the competition for similar conditions, the relations of the pursuer and the pursued, and mimicry, are set forth in abundance. The curious dorsal eyes of the marine slug *Onchidium* are described and figured, and an ingenious attempt is made to account for their evolution in relation to the pursuit of the *Onchidium* by the leaping-fish *Periophthalmus*. Prof. Semper is not blundering when he states that these eyes are constructed on what he calls "a type identical with those of the vertebrata." At the same time such a statement is very misleading, for these eyes differ essentially in their origin and structure from those of vertebrates, although having one superficial resemblance to the vertebrate eye in the fact that the retinal nerve is distributed to the anterior instead of to the deep surface of the retinal cells. This arrangement exists also in *Pecten*, contrary to Prof. Semper's statement that *Onchidium* is a solitary example of its occurrence in invertebrata.

As to mimicry Prof. Semper brings forward a new instance among land-snails where a Philippine *Helicarion* which sheds its tail (metapodium) and so escapes when seized by a bird or lizard, is imitated closely in appearance by a *Xesta* which has not the power of shedding its tail, but benefits by the reputation for elusiveness of the *Helicarion*. On the general subject of mimicry Semper does not consider the doctrine of selection adequate, but thinks it necessary to improve the current theory relating to it by some original touches. He has made the not very new discovery that "under some circumstances the most perfect and complete resemblance between two creatures not living associated may originate without its being referable to the selective power of mimicry, *i.e.* a protective resemblance." The resemblance referred to is of course a superficial one of colour or appearance of one part of the body, and not really "perfect" or "complete." From this he goes on to suggest that subsequently to this stage a necessity for protection may arise, and the previously-established resemblance may become protective to one or other of the reciprocally counterfeit organisms. On the strength of this suggestion he proceeds further to question whether natural selection has ever produced mimicry, and declares that some causes "must have availed to produce by their direct action an advantageous and protective change of colouring" in the first instance. Similar to this, he states, is the conclusion which is arrived at in each chapter of his book in reference to other adaptations besides those coming under the head of mimicry, *viz.* that natural selection cannot operate until directly transforming agencies have produced advantageous characters of a definite and obvious kind upon which it may operate.

With the whole of this reasoning, and especially with the statement that any such conclusion can be derived from the facts stated in earlier chapters, we disagree.

On the contrary, we maintain that natural selection operates upon advantageous variations which are exceedingly small, and do not, by an immense interval, amount to such coarse advantages as those assumed by Prof. Semper. Such small variations are incessantly caused by the action of external forces on the complex physiological units of the parents and by the action of those of one parent upon those of another. These causes of variation are not transforming causes, but produce irrelative and multifarious variations of small amount. It is upon these that natural selection acts. The existence of such variations, the power of selection to intensify them, and so to transform species and further the natural existence of a necessary selection, have been established by Mr. Darwin by an enormous mass of evidence. Prof. Semper, so far from having brought his reader in each chapter to a conclusion favourable to his views, has not adduced any evidence to show that natural selection cannot or does not act as taught by Mr. Darwin, and has moreover completely failed to adduce any evidence making it even probable that large changes of structure are ever effected by "directly transforming agents," of the very existence of which he can offer no evidence. Still less has he succeeded in showing that natural selection does or even that it could make use of such large changes—concerning which it is difficult to reason, since nothing is known about them excepting that Prof. Semper believes in them.¹

The supposed cases of minute resemblance without mimicry which are given by Semper are either to be explained as due to a protective resemblance to a third object, or as due to like advantages secured independently in each case by natural selection in a way which may become apparent when we have more ample knowledge of the particular cases, or lastly, as due to an accidental superficial identity in two things having absolutely no relations in common. To argue that the last account of the matter is the true one, and that the elaborate mimicry of insects is to be explained with the assumption of the frequent occurrence of such coincidences rather than by the doctrine of natural selection, is, it may be conceded,

¹ It is necessary to plainly and emphatically state that Prof. Semper and a few other writers of similar views (*e.g.*, the Rev. George Henslow in *Modern Thought*, vol. ii. No. 5, 1881), are not adding to or building on Mr. Darwin's theory, but are actually opposing all that is essential and distinctive in that theory by the revival of the exploded notions of "directly transforming agents" advocated by Lamarck and others. They do not seem to be aware of this, for they make no attempt to seriously examine Mr. Darwin's accumulated facts and arguments. The doctrine of organic evolution has become an accepted truth entirely in consequence of Mr. Darwin having demonstrated the mechanism by which the evolution is possible; it was almost unanimously rejected, whilst such undemonstrable agencies as those arbitrarily asserted to exist by Prof. Semper and Mr. George Henslow were the only causes suggested by its advocates. Mr. Darwin's argument rests on the *proved* existence of minute many-sided, irrelative variations not produced by directly transforming agents, but showing themselves at each new act of reproduction as part of the phenomenon of heredity. Such minute "sports" or "variations" are due to constitutional disturbance, and appear not in individuals subjected to new conditions, but in the offspring of all, though more freely in the offspring of those subjected to special causes of constitutional disturbance. Mr. Darwin has further *proved* that these slight variations can be transmitted and intensified by selective breeding. They have in reference to breeding a remarkably tenacious or persistent character, as might be expected from their origin in connection with the reproductive process. On the other hand mutilations and other effects of directly transforming agents are rarely, if ever, transmitted.

It is little short of an absurdity for persons to come forward at this epoch, when evolution is at length accepted solely because of Mr. Darwin's doctrine, and coolly to propose to replace that doctrine by the old notion so often tried and rejected.

That such an attempt should be made is an illustration of a curious weakness of humanity. Not unfrequently, after a long-contested cause has triumphed and all have yielded allegiance thereto, you will find when few generations have passed that men have clean forgotten what or who it was that made that cause triumphant, and ignorantly will set up for honour the name of a traitor or of an impostor, or attribute to a great man as a merit, deeds and thoughts which he spent a long life in opposing.

original and startling; but it involves a deliberate renunciation of the exercise of reason.

The translation of Prof. Semper's highly entertaining and really valuable and suggestive book has been remarkably well executed. Throughout great care has been taken to give the correct English equivalents for the German names of many obscure animals, and to preserve the sense of the original. At the same time there is not from beginning to end any trace of that awkward diction which sometimes infects a translation from the German. It is not too much to say that it is the best executed translation of a foreign work on science which has appeared for twenty years. E. RAY LANKESTER

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Movements of Plants

FRITZ MÜLLER, in a letter from St. Catharina, Brazil, dated January 9, has given me some remarkable facts about the movements of plants. He has observed striking instances of allied plants, which place their leaves vertically at night, by widely different movements; and this is of interest as supporting the conclusion at which my son Francis and I arrived, namely, that leaves go to sleep in order to escape the full effect of radiation. In the great family of the Gramineæ the species in one genus alone, namely *Strepium*, are known to sleep, and this they do by the leaves moving vertically upwards; but Fritz Müller finds in a species of *Olyra*, a genus which in Enlicher's "*Genera Plantarum*" immediately precedes *Strepium*, that the leaves bend vertically down at night.

Two species of *Phyllanthus* (Euphorbiaceæ) grow as weeds near Fritz Müller's house; in one of them with erect branches the leaves bend so as to stand vertically up at night. In the other species with horizontal branches, the leaves move vertically down at night, rotating on their axes, in the same manner as do those of the Leguminous genus *Cassia*. Owing to this rotation, combined with the sinking movement, the upper surfaces of the opposite leaflets are brought into contact in a dependent position beneath the main petiole; and they are thus excellently protected from radiation, in the manner described by us. On the following morning the leaflets rotate in an opposite direction, whilst rising so as to resume the diurnal horizontal position with their upper surface exposed to the light. Now in some rare cases Fritz Müller has observed the extraordinary fact that three or four, or even almost all the leaflets on one side of a leaf of this *Phyllanthus* rise in the morning from their nocturnal vertically dependent position into a horizontal one, without rotating, and on the wrong side of the main petiole. These leaflets thus project horizontally with their upper surfaces directed towards the sky, but partly shaded by the leaflets proper to this side. I have never before heard of a plant appearing to make a mistake in its movements; and the mistake in this instance is a great one, for the leaflets move 90° in a direction opposite to the proper one. Fritz Müller adds that the tips of the horizontal branches of this *Phyllanthus* curl downwards at night, and thus the youngest leaves are still better protected from radiation.

The leaves of some plants, when brightly illuminated, direct their edges towards the light; and this remarkable movement I have called paraheliotropism. Fritz Müller informs me that the leaflets of the *Phyllanthus* just referred to, as well as those of some Brazilian *Cassieæ*, "take an almost perfectly vertical position, when at noon, on a summer day, the sun is nearly in the zenith. To-day the leaflets, though continuing to be fully exposed to the sun, now at 3 p.m. have already returned to a nearly horizontal position." F. Müller doubts whether so strongly marked a case of paraheliotropism would ever be observed under the duller skies of England; and this doubt is probably correct, for the leaflets of *Cassia neglecta*, on plants raised from

seed formerly sent me by him, moved in this manner, but so slightly that I thought it prudent not to give the case. With several species of *Hedychium*, a widely-different paraheliotropic movement occurs, which may be compared with that of the leaflets of *Oxalis* and *Averrhoa*; for "the lateral halves of the leaves, when exposed to bright sunshine, bend downwards, so that they meet beneath the leaf."

CHARLES DARWIN

Down, Beckenham, February 22

Barometric and Solar Cycles

REGARDING one of the conclusions drawn by Mr. F. Chambers in his paper on "Abnormal Variations of the Barometer in the Tropics," and Dr. Balfour Stewart's remarks concerning the same in the first article of *NATURE* (vol. xxiii, p. 237), I and other meteorologists would like very much to know which side of the earth is to be considered the east, and which the west.

In other words, if waves of high barometer travel slowly from west to east, on what meridian do they commence, and is there any reason why they should commence on one meridian more than on another? The only reason that I can think of is that some meridians embrace more land than others; but in this respect the meridians passing through the centres of America, Europe-Africa, and East Asia-Australia are very much alike. Again, if barometric changes originate, say at St. Helena, and travel slowly eastwards, as Mr. Chambers supposes, they ought after several months to reappear on the meridian from which they started, but Mr. Chambers's paper gives no evidence of this whatever.

Dr. Balfour Stewart says it is unmistakably indicated by all the elements that the connection between the state of the sun's surface and terrestrial meteorology is of such a nature as to imply that the sun is most powerful when there are most spots on his surface. The barometric evidence, however, is all the other way.

Mr. Blanford, following up a suggestion originally made by the present writer, has shown clearly enough that the decennial variation of the height of the barometer has nearly opposite phases in the Indo-Malayan region and in Western Siberia, especially if the winter season, when the pressure is higher over Siberia than in South-Eastern Asia, be considered alone (*NATURE*, vol. xxi, p. 480). From Mr. Blanford's paper it is clear that the barometrical differences, on which the strength of the winds depends, are greater when the sun-spot area is small than when it is large.

The true relation between the variations of sun-spot area, solar radiation, and barometric pressure will, I feel confident, be soon discovered through the agency of the United States Weather Maps in the manner pointed out by you at page 567, vol. xxi., in discussing the United States Weather Map for July, 1878. It is there shown that in the middle of summer in the last year of minimum sun-spot, the pressure of the air was below the average over all the great continents, and above it over the neighbouring oceans. In India, it is true, the pressure was above the average; but then India is not Asia, but merely a narrow triangular peninsula surrounded on two sides by the ocean, and on the third by a broad zone of snow covered mountains which may be likened to an oceanic area as far as constancy of temperature is concerned.

Meteorologists will all agree with Dr. Balfour Stewart that "unexceptionable observations of the sun's intrinsic heat-giving power, if these could be obtained, would furnish a more trustworthy instrument of prevision than the sun-spot record." We may soon hope for a nearly continuous series of such observations, for, according to the last published Administration Report of the Indian Meteorological Department, a trustworthy form of actinometer is being sent to Leh, 11,500 feet above the sea, in the dry region of Tibet, where observations will be taken with it under the superintendence of Mr. Ney Elias.

Meantime we may perhaps adopt what is considered by Mr. Blanford the best criterion of the sun's heating power which can be obtained from ordinary meteorological observations, viz. the highest excess of the vacuum black-bulb thermometer above the maximum in shade for each month. At ten stations in India where comparable thermometers have been used since 1875, the mean maximum solar excess has been:—

1875	...	1876	...	1877	...	1878
67°·0		67°·2		68°·8		68°·1